AMERICAN SOCIETY OF CIVIL ENGINEERS.

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CLXXXII.

(Vol. VIII.—July, 1879.)

ON THE FLOW OF WATER IN RIVERS.

By DE VOLSON WOOD, C. E., Member of the Society.

PRESENTED AT THE ELEVENTH ANNUAL CONVENTION, JUNE 17, 1879.

Read July 16th, 1879.

At the Convention of the Society in June of last year, held in Boston, the eminent hydraulician, James B. Francis, reported the result of an experiment made by him, which showed that the water at the bed of a stream of water gradually rises to the surface. He placed a white substance, having about the same specific gravity as water, at the bottom of the stream, leaving it in such a condition that the moving water would take with it some of the substance; and he observed that the water thus colored rose to the surface at a point some distance below where it was placed. This, so far as I know, was the first definite statement of this mode of action in streams of water. In the discussion which followed, I stated that admitting the correctness of the principle stated by Mr. Francis, it followed that the water at the surface must necessarily find its way to the bed of the stream, and that this action

might enable us to account for the fact, now generally admitted, that the point of maximum velocity in the cross-section of a flowing stream is not at the surface, but at a point below the surface, generally between the surface and the half depth of the stream. My view, as then expressed, was that gravity acting upon the particles during their descent would accelerate, or at least would tend to accelerate, their motion; and that the action of gravity, combined with the motions impressed by the mutual actions of the particles, might account for the position of the point of the maximum velocity. This view I still believe to be correct, and I desire in the following paper to consider more in detail some of the principles herein involved.

The earliest experiments showed conclusively that the velocity at different points in the cross-section of a stream was not the same—that it was greater near the middle than at the sides of the stream, and greater at the surface than near the bottom. Improved apparatus and more careful experiments led to the statement that the maximum velocity was at the surface, and that it decreased with the depth, though not as a direct single function of the depth. Some assumed that the curve of velocities would be represented very nearly by a parabola whose axis was vertical and whose vertex was at some unknown distance below the bed of the stream (see D'Aubisson's Hydraulics). Most of the earlier experiments were made upon rectangular canals of comparatively small depths and of comparatively small beds—conditions not realized in the more important streams. and which, on account of the small dimensions of the cross section, rendered it impossible to determine by direct measurement the velocity at all points with sufficient accuracy to enable us to detect the true point of maximum velocity. It was in connection with experiments upon such a canal that M. Bazin, about the middle of the present century, concluded that the maximum velocity at the middle of a stream was at the surface, although he admitted that between the middle and the sides the maximum velocity would be a short distance below the surface (see Morin's Hydraulique).

Experiments upon rivers of considerable size have shown that the point of maximum velocity is below the surface of the stream.* Since the discovery of this principle, many writers attribute the loss of surface

* See Ellet on the "Mississippi and Ohio Rivers," 1853; Boileau, "Traité de la Mesure des Eaux Courantes," 1854; Darcy et Bazin, "Recherches Hydrauliques," 1865; "Report on the Physics and Hydraulics of the Mississippi River," by Captain Humphreys and Lieut. Abbot, 1861; Morin's Hydraulique.

velocity to the frictional resistance between the water and the superincumbent air. But this assumption was disproved by Humphreys and Abbot, by finding that when the wind was blowing down stream with the same velocity as the surface of the stream, or even with a greater velocity, the point of maximum velocity was still at a perceptible distance below the surface. Boileau was satisfied from his experiments that the diminution of velocity from the maximum to the surface could not be attributed to the frictional resistance of the air. He says: "We must seek for an explanation of this phenomenon principally in the mutual actions which bind the particles to each other, and in the oblique and rotary movements among the particles which result from these mutual actions and the unequal velocities of the neighboring particles." This statement, made a quarter of a century ago, contains sound philosophy, and furnishes a *general* explanation of several phenomena, the particular reasons for which it is not so easy to assign. For instance, it is not easy to explain why particular whorls are formed in water, or why they take a particular path, or why the water in a deep stream appears to boil, or why parts of a stream, in certain cases, flow up stream while the general declivity is down stream, &c.

The path of a particle in a stream of water of uniform *régime* is exceedingly tortuous. The experiments of Francis, cited at the beginning of this article, show that the particle leaves the bed of the stream and ascends more or less gradually to the surface, and as this may take place with all the particles, it necessarily follows that those at the surface must descend to the bed. We are not, however, dependent upon this experiment for establishing the fact. Any one, by observing a stream of say 5 or 6 feet or more in depth, can see that the surface appears to boil. This is caused by the ascent of bodies of water rising from some depth to the surface, and there spreading out upon the surrounding water. It thus appears that the particles, instead of moving among each other like filaments, go in bodies like streams of a few inches—or sometimes of a few feet—in diameter moving through the main body of the water. Or, possibly, these bodies are more like spheres, or other inclosed masses of water, passing from the bed of the stream to the surface, where they spread out, and afterwards form new combinations.

The motion here referred to is mostly in a vertical plane, but similar causes produce lateral movements. Every body projecting from the

shore into the stream deflects the water, causing a wave-like motion extending from the shore to, or past, the middle of the stream. The retarding influence of the sides of the stream will cause a lateral action, just as the resistance of the bed of the stream causes a vertical action; and as the water along the shore goes more or less gradually towards the centre, it follows that the water along the centre must move laterally outward to supply the place of that which leaves the shore. It is hardly necessary to note the fact that the lateral movement is not in a plane, nor that its intensity is small compared with the vertical action, although it is an element not to be entirely ignored in considering the general problem. Since both the vertical and lateral actions are induced by the forward movement of the water in the stream, we might expect that there would be a greater accumulation of water where the velocity is greatest; and such is known to be the case, the water being highest at that point in the transverse section when the velocity is greatest. The cause of this elevation is not due to the lateral and vertical actions, but rather to the fact that the lateral pressure of a fluid diminishes as its velocity increases.

It is highly probable that the vertical movements originate at the bed of the stream, for any irregularities, such as small asperities due to a sandy or gravelly bed, or sticks or weeds, or changes in the slope of the bed, will cause the water either to ascend or descend. Since the density of the water is uniform throughout, it will, when once deflected upward in its course, continue to rise until it reaches the surface unless arrested by the downward movement of other bodies of water. We have reason to believe that this action is continually going on even where its presence is not easily detected by direct observation. It must, however, be kept in mind that these vertical and lateral movements are only secondary to the longitudinal one, and hence enter, not as a principal, but as a modifying cause. Granting these premises, it follows that the apparent, or measured, velocity at any point of a stream of water is the resultant of the volocities of many particles, and is not necessarily the actual velocity of any of them—some particles moving laterally in reference to the measuring apparatus, some slower and some faster than the measured velocity.

As any body of water is rising vertically in the stream, its motion is retarded by gravity, and as it starts with a slow velocity at the bed it acts upon all the water through which it passes, tending to retard the

velocity of the stream, while the stream by acting upon this mass tends to impart to it the same velocity as the stream. There is a tendency towards a common velocity between the stream and rising mass, but observation shows, as cited above, that, in many cases, the motion of each is distinct from the other. Such is particularly the case where the surface has the appearance of boiling. I do not now see how the ascending mass can account for the maximum velocity being below the surface. But the descending mass will have its motion accelerated by the action of gravity, and if there were no resistance, this acceleration would go on till the mass reached the bottom, like the motion of a projectile in a vacuum. But as the mass descends it comes in contact with water which is moving slower than itself, and hence its velocity will be retarded. But we notice particularly that for a certain fraction of the depth the velocity may—when the vertical action is not considered—be *nearly* uniform, hence when the descending mass passes through it with a tendency to acceleration it will at first impart to the surrounding water an increased velocity, and this increase may be sufficient in many cases to cause the velocity below the surface to actually exceed the velocity at the surface. We cannot, however, at this stage of the theory, assert that this is the only, or even that it is the chief cause of surface diminution of velocity. It is possible that the lateral surface movement, caused by the action of the sides of the stream, as previously stated, is quite as important an element as the preceding.

Having indicated that the lateral and vertical movements may account for the point of maximum velocity being below the surface, we will suggest that a more critical study of the results of experiment and observation may confirm or modify this view. Thus, in a rapid, deep stream we would expect the point of maximum velocity to be deeper when the bed is rough and rocky than when it is smooth and regular. A study also of streams of different widths, but of the same depth and general character of bed, would enable us to judge more accurately of the effect of lateral movements.

These views also enable us to explain certain other phenomena. For instance, it has long been known that a boat of considerable dimensions will float down a stream more rapidly than the surface water. That this is the fact is known by observing that the boat floats faster than chips and small pieces of boards in the immediate vicinity of the boat. The latter objects being small will be resisted more in proportion to their

weight than larger ones, and hence will move with nearly the same velocity as the water in which they float. Some have attempted to explain the rapid motion of the boat from the fact that it extends downward into the more rapidly moving elements; but this explanation does not appear to be sufficient, for the velocity of the boat, in some cases at least, exceeds the maximum flow of the water. We submit the following explanation: The boat, in floating down stream, is partly in the condition of a body moving down an inclined plane, and, as such, may be considered as having a velocity independent of the stream. If we could conceive of the water in a stream having no velocity and offering no resistance (or only a very slight resistance) to the movement of the boat, the boat would (or at least might) slide down the plane. But in the actual case, the water does move, although it does not, as we have seen, go *directly* down stream. The boat slides (so to speak) down stream, but the particles of water pass from the bed to the surface, and from the latter to the former; also from the side to centre, and from the centre outward, so that while the boat is taking a direct line down stream the water on which it is gliding is wandering about, taking comparatively long journeys.

We see from this that the *laminar* theory (so to speak) in which it is assumed that layers next to the bed of the stream remain in contact with it, and that the successive laminæ retain their relative positions is no longer tenable. Also that the supposed frictional resistances between consecutive surfaces will not account for all the varying velocities. Also that these lateral and vertical movements have an important bearing upon the phenomenon of "back water." Also that much of the so-called "frictional resistances," or resistance due to the adhesion between the particles, may be explained by these movements.

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DISCUSSIONS AT THE ELEVENTH ANNUAL CONVENTION ON PAPER CLIX.

ON THE THEORETICAL RESISTANCES OF RAILROAD CURVES.*

By S. WHINERY, C. E., and J. F. FLAGG, C. E.,
Members of the Society.

S. WHINERY.—The larger part of the criticism and discussion of the paper presented by me (No. CLIX), that has come to my notice, is contained in private letters to me from friends and those interested in the subject of which it treats. The great diversity of opinions expressed shows that the subject is a complex one, and that the ideas held by many of us are neither clear nor exact.

I am confirmed in the opinion that theoretical and analytical investigation is likely to be of great value in reaching a final solution of the problem of curve resistance, and devising means to obviate it.

If it is objected that final and absolute results can never be reached in this way, we may answer that experiment has not, so far, given satisfactory and conclusive answers to the problem. Together, theory and experiment may furnish the desired solution.

In reviewing the discussion offered, I shall not confine myself to that published in the "Transactions," but will refer to such private criticism as seems worthy of attention.

The discussion by Mr. Chanute, contains, probably, the best summary in the English language, of the results of experimental researches on curve resistance, and is of great value to engineers.

* Vol. vii., page 79. April, 1878.

As was to be expected, the greater part of the criticism brought to my notice was directed toward the vexed question of coned wheels. Mr. Chanute, in referring to this part of the paper, misapprehends the conclusion arrived at by me as to the effect of coned wheels on curves of radii not falling under certain limits. The conclusion is stated at the top of page 91 of the April, 1878, Transactions, in this language: "Hence, we are forced to conclude that the coned wheels of a car will adjust themselves to any curve whose radius is not less than the altitude of the cone, of which the wheels in the position of greatest difference of diameter, forms a frustum;" and the reasons which lead to this conclusion are given on the page preceding. As yet, I am not able to see that these reasons and the conclusions to which they lead are erroneous, unless it be in the omission of the element of parallelism of axles from consideration. Whether this omission is permissible or not is a question that admits of debate. On a curve of the radius stated in the paper (1,887 feet), if the axles were placed in a radial instead of a parallel position, they would be .016 feet nearer together at the inner than at the outer wheels. If this amount be divided between the four bearings of the axles it will be about $\frac{3}{64}$ inch at each; now, there are very few trucks that will not allow of this amount of play in the bearings of the axles or the boxes.

But admitting that the axles are held rigidly parallel, the power required to make the wheels follow the curve is not great. If a four-wheeled truck, having its axles fixed in a parallel position, and having the wheels on one side smaller than those on the other side, be placed on a plane surface and rolled forward, it will not move freely. Either the smaller wheels will be slipped on the plane an amount equal to the difference of circumference of the larger and smaller wheels, and the truck will move forward in a straight line, or the larger wheels will be slipped latterly, in which case the motion will be deflected toward the side with the smaller wheels, and the path of the trucks will be a curve. The force required to slip the smaller wheel longitudinally, and the larger wheels laterally, is probably about the same; but the former acts upon the truck through a lever arm equal to the gauge of the track, and the latter with a lever arm equal to the wheel base of the truck, and where the wheel base is longer than the gauge, as is always the case in practice, the truck will move forward in a straight line; but if the truck

were so constructed that the wheel base was shorter than the gauge, the conditions would be reversed, and the truck would move in a curve whose radius would have a constant ratio to the difference in circumference of the larger and smaller wheels.

This fact, which the writer has verified by experiment, seems to have been overlooked by other investigators who have constantly maintained that "a car truck will always move in a straight line whether the wheels on one side be of the same or a different diameter from those on the other."

Assuming that my position is correct, it is clear that if the wheel base and the gauge are just equal the lever arm of each resistance would be equal, and a slight impulse would determine the direction of the motion within certain limits. In the ordinary car truck with a wheel base of 6 feet, the tendency to move in a straight line is to the tendency to move in a curve as 6 feet is to 4 feet 10¼ inches. Hence the conclusion that the force required to guide such a truck around the curve specified is not of much magnitude, and that the effect of parallelism of axles has been generally overestimated.

As to the resistance caused by coned wheels on straight track, the conclusions of the paper are probably erroneous. I had hoped that Mr. Chanute would be able before this meeting, to extend his experiments on the area of the surface of contact between wheel and rail, to ordinary car wheels. It will be remembered that his former experiments were with engine driving wheels whose steel tires are not allowed to become so much worn as the ordinary car wheels contemplated in the paper. His experiments show that the line of contact between wheel and rail, across the latter, is for engine drivers "partly worn," ¾ inch each, and in the one experiment with a wheel "much worn," this line was two inches in length. The average condition of ordinary car wheels is probably not far from a mean between the conditions "partly worn," and "much worn," and these experiments would seem to confirm the value assumed for this line in the paper.*

* Mr. Chanute handed me, a few minutes before the above was read, a table giving the results of a large number of experiments, made very recently, to determine the area of the surface of contact between ordinary car wheels and the rail. Several of these were made with wheels worn so concave that they made two surfaces of contact instead of one.

Rejecting these latter, the average of twenty experiments gave a surface of contact 0.31 inch by 0.36 inch, the latter being the average length of the line of contact across the rail. These clearly prove that the length assumed for this line in the paper is erroneous.

The most valuable criticism of this part of the paper that has come to my notice, is contained in a private letter from my friend W. A. Gunn, member of the Society. Referring to the theory set out in the paper, as to the resistance caused by this line of contact, he says:

"This theory might be correct if the top of the rail was a plane; but the top of the rail is a convex surface, generally; and the radius of the surface of the rail most used, is not far from the radius of the ordinary car wheel. The contact of these two cylindrical surfaces would be a point, if both bodies were perfectly rigid; but not being so, the weight forcing them together, will make a line of contact both longitudinally and transversely of the rail; and it is probable that the line is longer in the direction of the length of the rail, than across it; because the rail gives under each wheel while the wheel is more rigid. The surface of contact would be something like that shown in Fig. 1, *A B* being across the rail, and *C D* in the direction of length. Now, of course, any movement of these surfaces, as a rule, will take place around the point of greatest friction, *E*, as a center. I think it will make no difference in results to estimate the effect of the friction upon one line of contact as *A B*, or on both lines, *A B*, *C D*. But considering the movement of the surfaces in contact on the line *A B*, about the *center*, instead of about the *end A* (as in your paper), would reduce the friction one-half, as shown by constructing triangles to represent it, as in Fig. 2. The sum of the areas of the triangles *A E C* and *D E B* is only one-half the area of *A B F*. Therefore, the friction would only be one-half as much as you have concluded, assuming that the top of the rail were a plane. But a comparison of figures 1 and 2 will show that the portion of the surface of contact nearest the extremities of the line, where the friction is least, move over the widest part of the triangle which represents the movement of the surfaces of contact. This would again reduce the friction at least one-half, and it would therefore not exceed one-fourth the amount you have figured out for it."

FIG. 1.

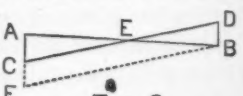


FIG. 2.

This view seems to be, with perhaps some slight modification, correct.

The position taken in the paper, that in passing around a curve, the rear inner wheel of a truck presses against the inner rail with the same force that the outer leading wheel presses against the outer rail, has been vigorously combatted by a number who have discussed the matter; several of them claiming to have demonstrated by experiment that the rear pair of wheels pass around the curve in a symmetrical position; that is, that the flanges of the wheels keep a position equidistant from their respective rails. Theoretically the position taken in the paper would undoubtedly be correct, if the wheels fit the rails accurately; that is, if there was no play between the flanges and the rails; but this is not the case in practice. With one inch play between the flanges and the rail, the longitudinal axis of the truck, must, when the outer leading flange is pressing the rail, make such an angle with the tangent to the curve, at the middle of the truck as to carry the inner rear flange away from its rail, in spite of its inclination to press it. This diagonal position of the truck on the track gives rise to another element of resistance nearly equal to that which it obviates, and the result stated in the paper is not much altered.

I have no doubt but this possibility of a truck's assuming a diagonal position on the track, taken in connection with excessive play between wheels and rails, will account for the violent oscillations and concussions observed on straight track, as well as curves, on some roads. Under such conditions, a truck may be thrown into a position on the track, where the angle between the track and the truck, as explained above, may be so great that the coning of the wheels may not be able to correct it before the forward flange is thrown violently against the rail. Whether the coned wheels are primarily responsible for these oscillations, I am not prepared to say.

I am not of opinion that it is either necessary to accuracy, or desirable for convenience, to combine formulæ Nos. 5 and 7 of the paper into one equation, as suggested by Mr. Emery. I have not taken the time to work out fully Mr. Emery's diagram (Fig. 1 of his discussion), but I do not believe that computations made in accordance with it would differ materially in result from those given by the sum of the results of the two formulæ above named.

Mr. Emery's point in reference to the force required to turn the car-body through the angle from one tangent to the other, appears, at first sight, well taken; but a little consideration will show that it is of no im-

portance. If we suppose the car-body resting at its centre of gravity on a point upon which it turns without friction, and neglect the resistance of the atmosphere, any force, however small, applied to either end, perpendicular to its axis, and in the plane of the horizon, will cause it to move, and if the force is then removed, the car will continue to revolve until opposed by a force equal in amount to the one that put it in motion. In actual practice, however, it would be necessary to overcome the friction of the support on which the body turns, and the resistance of the atmosphere. In the case of a car, with two bogie trucks, entering a curve, the leading truck would be revolved on the support through an angle equal to 90° minus the acute angle between the radius to the curve and the chord passing through the two supports of the car, and the rear truck would be revolved an equal amount in the opposite direction; but as soon as the car was fully upon the curve there would be no more motion, and, consequently, no more friction on the supports so long as the curve remained of constant radius, and, except for the very small resistance of the air, the car-body would continue to revolve without the consumption of force, until stopped by an equal opposite force, when the car should leave the curve; and, so far as track resistance is concerned, the one would counterbalance the other. There only remains the slight resistance offered by the air, which is too small to be worthy of attention.

In reference to the question of loss of power at each coupling of the train, I would refer those desiring to investigate the matter more fully, to "Coxe's Weisbach's Mechanics," Vol. I, § 152, page 283, where a full demonstration of the principle will be found.

It will be noted that the coefficient of friction is an important factor in the formulæ given in the paper, and any change in its value would modify the results accordingly. Whether the value assumed in the paper for this coefficient, is even approximately correct, is an open question. The recent experiments in England seem to prove, that the coefficient of friction decreases very rapidly as the velocity of the rubbing surfaces increases. If this is true, it would necessitate corresponding modifications of the formulæ given. Besides, it is not at all certain that the coefficient of friction is the same at a given velocity, under the various conditions of friction. Thus, the coefficient of friction for the longitudinal slip of a wheel, may be quite different from that of the same wheel, for lateral slip, while rolling forward.

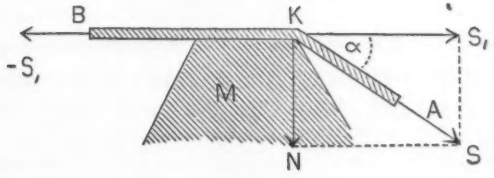
J. F. Flagg.—I desire to say a few words only in regard to section 8 of the interesting paper of Mr. Whinery, in which he deduces a loss of tractive power from the line of tractive force forming a funicular polygon.

This loss of power is disputed by Mr. Emery (Vol. VII, p. 104), but is reasserted by the author of the paper in the "Railway Gazette" for April 18th, 1879, where he endeavors to maintain his position by a reference to "Weisbach's Mechanics."

It seems to me that the author is a little at fault in his dynamics. *What work* does this lost power do? He says that "at each angle the tractive force is decomposed into two components, one of which is expended in drawing the following car along the track;" but in what work is the other component expended?

He adds, "it *tends* to move the angle itself towards the centre," but does it *do* any work of this kind? I think not. This radial force, undoubtedly, produces friction, which consumes a portion of the tractive power; but throwing friction aside, I fail to see any proper decomposition of forces that can show any loss of power from this cause.

The author refers to page 283 of Coxe's Weisbach. There the initial force upon the cord, on the polygon, is decomposed into two, one of which is perpendicular to the other, or following tension in the cord. Why should an assumption like this be made? If, instead, the former component is taken to bisect the angle formed by the cord—or in case of the railroad train, is taken normal to the curve—there will be no loss found in the decomposition; and this is the only natural way to



obtain the relation of the several forces. If section 152 (page 283) of Coxe's Weisbach is correct, by reversing the point of application of the power, from *A* to *B*, the strain upon the rope would be *increased* at the opposite extremity, an absurd result, unless other conditions are introduced. If the support, *M*, rests upon a frictionless surface parallel to

the resulting tension ($-S$,) on the cord, with the condition added that there should be a complete static equilibrium including the support, then, of necessity, this second component (N) must be perpendicular to this surface, and consequently to the resulting strain. Certainly no such conditions hold for the railroad train.

I cannot appreciate the value of this section in Wiesbach's Mechanics, the mode of decomposition of forces seems so arbitrary and unnecessary; it is entirely wanting in the earlier American edition, edited by Walter R. Johnson. And if Mr. Whinery will refer to the following chapter of Coxe's Weisbach, pp. 356 to 358, in which a rope is stretched, in the form of a funicular polygon, over several sharp angles, he will see that here Weisbach has deduced the final strain upon the cord, from the initial one, simply by obtaining the friction at each angle, and subtracting the total friction from the initial force, utterly ignoring the matter quoted from p. 283; and the example of a chain stretched on the circumference of a fixed wheel (page 359), approximating pretty nearly the case of a railroad train, is also worked out with a loss from friction alone.

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DISCUSSION AT THE ELEVENTH ANNUAL CONVENTION ON PAPER CLXXII.

THE USE OF COMPRESSED AIR IN TUBULAR FOUNDATIONS,

AND ITS APPLICATION AT SOUTH STREET BRIDGE, PHILADELPHIA.*

By F. COLLINGWOOD, C. E., Member of the Society.

There are several things in Mr. Stauffer's paper which I wish to comment upon briefly.

The first is as to the particular arrangement of the joints in the cylinders. If I understand the drawing aright, interior flanges were separated one-eighth of an inch, when the exterior portions of the cylinders proper

* Vol. VII, page 297, October, 1878.

were in contact. Now, if the packing was always made thick enough to insure that the exterior portions did not touch each other, no harm could ensue. But if they were brought in close contact, a bending or tearing strain will be set up in the angles of the flanges, which will be so much in addition to the strain caused by the upward pressure of the air, and may become dangerous.

A case in point occurred in the sinking of some cylinders for a bridge near Fall River, several years since. I have no details at hand, but I believe the flanges of the air lock were 2" thick; the joint leaked air, and men were sent down to tighten up the bolts. Soon after, the air lock was blown off and several men lost their lives.

The locks for the Brooklyn caisson of the East River bridge had perfectly plain flanges, and were packed with rubber rings. When the locks were used temporarily on the New York caisson for filling up the water shafts, pig iron and lead were piled upon them, as they were not considered safe for the extreme pressure then required.

In reference to the doors of air locks, two precautions are desirable: First, that the holes in the hinges be elongated vertically, so that as the pressure comes on it can compress the rubber at the hinge, as well as elsewhere; second, that a thoroughly efficient and easily-worked arrangement be provided for hoisting up the doors to the locks. This must be such that a considerable pressure can be exerted aside from merely lifting the door to place, and so secure a perfect contact, as the rubber gets hardened by use, and will allow the air to rush past it as fast as it enters or escapes from the lock. A person who has once been imprisoned in an air lock will not willingly be subjected a second time to the same ordeal.

The paper mentions 3" diameter as the size of the equalizing pipes. It does not state whether these were opened full or not when men were passing through the lock. The physiological effects of such a rapid change of pressure as this would cause, could hardly fail to be serious. Two cocks, one inch diameter each, were used in the caissons of the East River bridge, and these openings were somewhat too great when the pressure was extreme. The heat and pressure on entering, or the cold and damp when leaving, are not pleasant experiences; but it is better to endure them a reasonable time, and avoid the possible injury to health otherwise caused.

It is somewhat remarkable that the air pressure was not used also for removing sand and mud, as well as for water.

The use of a straight pipe without water jet has been found very effective in removing sand, and is certainly more economical when the material is suitable, than the removal by bags.

From our experience in the caissons here, I have become convinced that all material can be removed more cheaply by compressed air than by any other method, where the caissons are of any size. No better method can be devised for *sand* than a perfectly straight pipe, without water jet or any other accessories. We have removed sand by a 3½ inch pipe, as fast as all the men that could work around it conveniently could shovel it to the pipe, say 20 yards per hour.

Where there are boulders, clay, or stiff mud, the air can be made to lift a cage, acting as a piston in a continuous cylinder which is connected with the atmosphere above (the cage containing the load to be removed). This system is now in successful operation in the Epinac coal basin, in France. Coal is lifted there from a depth of 2 000 feet, at a cost considerably less than by the usual method by cables. The miners all ascend and descend by the same apparatus.

The details for the application of this system to a caisson would be different, but I can see no difficulty in working out a scheme which shall be safer than a water shaft can possibly be made, which shall give a positive lift, free from the often ineffective working of a dredge; and which shall use no more air than ought in any case to escape, to keep the air in the caisson of sufficient purity.

The Epinac system is described in "London Engineering," of May 2, 1879.